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THE INADEQUACY OF "NATURAL" REALISM

A FEW years ago idealism in one form or another seemed to have definitely won the day in philosophy. But very recently the realistic heresy has sprung up again, and it is now rampant at Columbia, at Harvard, and at many another American university; it is even gaining ground in orthodox England. This contemporary movement, called by some of its leaders "natural" or "naïve" realism, is in large measure a reaction against the excesses of absolute idealism; the fathers have eaten sour grapes and the children's teeth are set on edge. Realism calls a halt to the building of metaphysical air-castles, and bids us keep our feet on *terra firma*. To all who long to see philosophy forsake irresponsible speculation and cleave to a surer if slower procedure, it is a welcome change. But just what *terra firma* is, how we may most accurately describe it, in the large, is not an easy matter to settle. In the opinion of the writer of this paper "natural" realism gives us an inaccurate and inadequate account of the facts, and must be supplanted by a critical realism which shall be based on the insight which idealism has contributed to philosophy.

There are, as a matter of fact, several mutually incompatible theories in the field, each of which purports to represent the instinctive realism of the natural man. These theories are not always kept distinct, and the difficulties in the way of one theory are often met by an unconscious shift of base to a point of view which is really quite different. The particular type of "natural" realism here opposed may possibly, when the full light of day is let in upon it, find no champions. So much the better, then; we shall have got out of the way a theory which, even if not consistently held by any of our contemporary realists, figures largely in their writings and gives much of its plausibility to their position.

This particular type of realism, then, is that most "natural" of all realisms, the belief that the very data which we have in experience slip out of experience and continue to exist, with the same qualities, when no one's experience includes them. It would seem to be the

belief indicated by Professor Montague's contention, agreed to by the notable Six, "that things known may continue to exist unaltered when they are not known."[1] It is the belief that the "this" which the idealist calls "content of experience" is the very "object" which the physicist speaks of as "out there," as the cause of our experience. By a critical realism, on the other hand, is meant a realism which holds that the "this" of experience, the object perceived, is numerically and qualitatively different from, and exists later in time than, the thing-in-itself "out there" which causes the experience.

The critical realist has much in common with idealists of the stamp of Berkeley and Mill. Both idealist and critical realist believe that these data which they have and describe exist only as elements of this particular experience—whereas to the "natural" realist the data of experience are permanent existences, now in some one's experience, now in no one's. It is a further consideration which parts the critical realist from the idealist, the latter holding that the cosmos is merely a construct, that there are no "absent objects" save in the sense of "permanent possibilities of sensation," while the former believes in a cosmos which really exists, in the same sense in which this "now" of experience exists. The difference between the natural realist and the critical realist is, then, that the former's universe is made up of the very "thises" of human experience, eked out with other contiguous "thises"; whereas the latter's universe is made up of realities which are pictured (to some extent) by the "thises" of experience, but lie beyond them. To the former, a man's experience when he looks at the starry heavens really includes a large percentage of the cosmos; to the latter it is but a momentary phase of this particular human experience, which bears the same relation to the universe as the man's body, or brain, does to other physical things. To the natural realist there is just this world that we see and touch, with our individual experiences flitting about in it like search-lights, illuminating portions of it that preexisted. To the critical realist all the things-I-see-and-touch are but visual-and-tactile-experiences within my total experience; each total experience is a microcosm, surrounded by a sea of things-in-themselves which never come within human experience at all; the macrocosm is much bigger than the natural realist imagines, it includes the separate streams of consciousness, with their subjective and objective experiences, and in between them a world of ejective realities which those reduplicated objective experiences more or less fragmentarily picture.

It must be confessed that the *prima facie* plausibility is with the natural realist, and the label of his system therefore legitimate. Our

[1] "The Program and First Platform of Six Realists," this JOURNAL, Vol. VII., p. 396.

contemporary realists never tire of telling us that their view "is the natural, instinctive belief of all men,"[2] and frankly shift the burden of proof to the shoulders of the sophisticated critic who would deny the belief of the "natural man" whose heart is pure of metaphysics. Now some of us are strongly inclined to suspect that your "natural man" is hopelessly unanalytic, and that if philosophy has any useful function in the world it consists largely in correcting his notions and making distinctions where his vague and intermittent reflection has discovered none. Moreover, the "natural man" is an unreliable witness to put one's trust in; he has been made sponsor to not a few theories for which we may well doubt his willingness to be responsible. At the outset he gives his unguarded assent, but when close inspection reveals all that the theory implies he backs out. The writer's experience with such "natural men" as he has caught is that when the implications of "natural" realism are relentlessly unfolded, they reject it with horror. Whither will the natural realist turn, then, if his own witness bewrayeth him?

It is significant that reflection has never been long satisfied with "natural" realism. It is precisely the difficulties in this obvious view that have precipitated many philosophic struggles; there are grave reasons why we must pass on beyond the surface look of things if we wish to construct a world-picture compatible with all the facts. To the "natural man's" assertion that we have our world in common—which is the contention of the "natural" realist—we have to reply, How do you know that we do? And we can go on to say, There are cogent reasons for denying that we do, in the strict sense. We *live* in a common world, but each of us is shut off from the rest of the world by the wall that surrounds his own consciousness; our visual and tactile experiences are within that wall and only *represent* what is without. We do not, strictly, have any "this" in common. The reasons for making this denial we shall proceed to note.

I

When we examine our *data,* we find that what we have at any moment, what we then know at first hand to exist, is not a world, but a very limited *field.* We have, *e. g.,* one side of a room, a bodily pain, a wish, and a vague background of other elements; and we have these things *existing together.* Whatever else may exist beyond this little circle of reality is really beyond it, is shut off from it, does not exist in that relation of felt togetherness with these elements in which they exist with one another. It is, partly at least, because of this primary fact that the conception of consciousness has been framed. Consciousness is this little field of reality that is present here, in

[2] *Ibid.,* p. 398.

contrast with a bigger world believed in beyond it. Or, if you please, consciousness is this peculiar relation, that of *felt togetherness,* which exists between these few data here. Whatever may be beyond this little field does *not* exist in this relation to the elements within the field, and can be proved not to. For the relation is precisely that of being *felt* together; and if those other elements are not felt together with these, then they do not exist in that relation with these. Your wishes and bodily pains, *e. g.,* are not felt together with mine; they exist in a separate field. I may speak of knowing your mind, but I can only mean that I know more or less *about* what is in your mind. In a sense I may say I have the "same" feelings. But that must not blind us to the simple primal fact that those very feelings which *you* are having—your bodily pain, your wish—are not felt with, do not exist in this relation of felt togetherness with, my pains and wishes, as my pains and wishes do exist with the other simultaneous elements in my field. It may be said that the absence of perceiving a relation is not the same as perceiving the absence of a relation. But that objection ignores the peculiar nature of this relation, which is nothing but that of being *felt* together. It is not the perceiving of a relation, but the existence of the relation, that is in question.

Suppose, then, you and I both say, "I see this tree." I have a "this" within my field which I call "this tree." It exists in the relation of felt togetherness with all the other simultaneous elements in this field—with, for example, a wish of mine. You likewise have a "this" within your experience which you call "this tree"; it exists in the relation of felt togetherness with a bodily pain of yours. Now the "this tree" in my field does *not* exist in the relation of felt togetherness with that bodily pain—as it *does* exist in that relation with my wish. The difference here is ultimate and indisputable, can be verified by any simple act of introspection. And if it is granted, the theory of natural realism is definitely refuted, by the logical canon of identity. The "this tree" in the one field exists in the relation of felt togetherness with a certain bodily pain; the "this tree" in the other field does not exist in the relation of felt togetherness with that bodily pain. A "this tree" that is in a certain relation can not be a "this tree" that is *not* in that relation. Therefore they are not the same "this tree" element. The "this" of your experience can not be identical with the "this" of my experience; they are two experiences, similar no doubt, but numerically different, one in your field, in your experience, in your consciousness (as you prefer to put it), the other in mine. The two fields do not overlap and include common elements; the world is, as the idealist maintains, a world of separate microcosms.

The natural realist and the absolutist have much in common—sworn foes as they are. They take common cause here, and maintain that though *I* do not feel this "tree" element with the bodily pain, they nevertheless are felt together by you, or by the absolute. Leaving out the implications of personal pronouns and capitalized absolutes, this means that though a felt togetherness between these two elements doesn't exist here, it exists somewhere else. But that it doesn't exist here, as a part of the felt togetherness of this field, is what we are affirming. There exists here and now a felt togetherness that extends so far *and no farther. This* field of felt togetherness does *not* include that pain. There is no boundary anywhere within this field; what is anywhere within it is in the felt together relation with what is anywhere else within it. But there *is* a boundary *about* it, that shuts out that pain. If there were not, all the rest of your field would be felt together with my field—our two consciousnesses would coalesce, we should be for the time one person. What need, then, of laborious mind-reading or elusive telepathy? The fact of this wall, this psychological horizon, this boundary to a field, is a perfectly definite fact, not to be glossed over. Such a wall exists between "this tree" No. 1 and that pain; it does not exist between "this tree" No. 2 and that pain. Psychology has not been talking in meaningless terms in speaking of my sensation and yours. I have avoided this phraseology, which might be deemed partisan; but the facts stand out nevertheless. The world will not be squashed down so small as the natural realists wish; there remain my walled-in field and yours and everybody else's, which never overlap or intersect.

II

This truth comes out still more vividly from a consideration of the *qualitative* difference between my "this tree" experience and yours. Data which are qualitatively different can not be numerically identical; and no "this" in my experience can ever be exactly the same, qualitatively, as any "this" in your experience at the same moment, could not be unless our bodies were exactly alike and occupied the same place in space, *i. e.*, in idealistic terms, unless *every* element in our two experiences were identical; in which case we should be one and the same person.

My present "this tree" may be, for example, a small, indistinct bluish object, while yours may be simultaneously a large, distinct gray and green object. A small bluish "this" *is* not a large green and gray "this." There is of course a practical sense in which we may call them different "aspects" of the same "object," the "object" being thus a construct. All of these "this tree" experiences have the same *function* in our experience, and according to

critical realism represent one thing-in-itself in the real world. But these various data are not the same datum, they are very different existences. So when I have that tactile experience of roughness, roundness, and hardness, etc., which I also call "this tree,' I have a datum utterly disparate from the visual "this tree." The two "thises" go together, the one always being a sign of the other; they have, again, the same function, occupy the same point in the conceived objective order; but they are totally different data. They may all be thought to exist, as the natural realist believes they do, permanently and independently of their togetherness with the rest of my experience; but *they would not be the same thing.* It is their function that is identical, not their being. What exists is a vast number of qualitatively different "thises," evanescent according to our theory, permanent according to natural realism; on any theory we only *have* them as evanescent, and must recognize that no two of them are exactly alike.

If we believe, then, that each of these "thises" that we have in experience is a permanent existence, we have a marvelously multiplied world. A thousand, a million different "thises" permanently exist as "this tree"! Disparate as they are, they can not be squeezed down into one simple object, and we have a world reduplicated *ad infinitum.* On our theory the really external objects, *i. e.,* the ejects, are simple existences, and these millions of "thises" exist merely (as we discover them) as transient bits of our human experience. But just as the absolute, if it is to include all the contents of all our minds, must contain a million reduplications of "this tree," so nature, on the theory we are combating, must contain a million trees where we thought there was one. Such a reduplication, the literal carrying-out of the "natural" realist's contention that "this" —this very "this" of experience—exists permanently, is repugnant even to the view of the "natural man," who, if he unwarily agrees with the "natural" realist's first assertion, agrees rather with us when the implications of the two theories are shown. We give him a simple, homogeneous external world; "natural" realism does not.

III

Among the other possible objections to natural realism there is one that may be briefly mentioned here—the time objection. When I look at a star, the critical realist expresses the facts as follows: Within my field of experience (or consciousness) exists this visual element, a-twinkling-point-of-light-out-there. The real star, the star-in-itself, is separated by a long succession of events (pictured by the million-mile-long ether line of the physicist) from this experience-of-a-twinkling-point-out-there. Suppose the star burned out (or

whatever stars-in-themselves do that we call burning out) a thousand years ago; this experience of mine is nevertheless a reality now. But according to natural realism it is the real star which my experience now wraps around and includes, not a something which represents it. That is to say, my experience now includes something which (unless we reject the conclusions of astronomy) does not exist now, has not existed for a thousand years! The qualitative difference between this twinkling-point-of-light and the real star, so vast and complex, should have taught us that the one is but a picture or sign of the other; the gap between their existence in time should be conclusive evidence.

Of course, what is true so strikingly in the case of a star is true less strikingly, but none the less true, in the case of all objects. The "real" object always exists earlier in time than the perceived object, which we hastily assume to be the "real" object, yet which is really but an element in our own experience, and not the object, or eject, which exists in and for itself.

IV

We have been arguing so far about the world of objects, or objective experiences; meeting the natural realist on his own ground, and seeking to show him that we have more of a reduplication than he realizes, and that if this reduplication is a matter not of evanescent existence in human experience but of permanent existence, we get not a simple, homogeneous world, but as many different *worlds* as we have *experiences.* Which is the better explanation of our experience of objects—a million streams of human experience, with a sea of simple things-in-themselves in between (so to speak), or a million worlds?

We may now point out, however, that "natural" realism is at its best when talking of physical objects; the theory was obviously framed to explain them. It proves much more inadequate when we wish to explain the "subjective" elements in experience—wishes, hopes, intentions, dreams, ideas, memories, and the rest. Now these elements of experience are perhaps as numerous and perhaps as important as the objective elements; and until an explanation of their existence is forthcoming, this theory is much less plausible than our critical realism, which offers a consistent explanation of their origin and relation to the objective elements. What sort of existence can a wish or a memory have outside of a conscious experience? Various fantastic theories have been put forward by the natural realists to account for these "inner" experiences; not having any observable place in the spatial world, they are relegated, according to some accounts, to a non-spatial world which simultaneously exists, elements from both worlds joining together to form experience. How this

taking partners occurs remains vague. How, moreover, in a world of objects experience of those objects can come in, where it comes from and goes to, is a mystery. This peculiar relation of together-ness-in-a-field, which is consciousness—how does it come to exist in a world of spatial and temporal relations? We have not space here to describe how critical realism explains these important facts; we shall merely assert that it has an explanation, and remind the "natural" realist that until he has one his theory is far from rounded out to such a dealing with the whole situation as shall command our attention.

According to critical realism, our fragmentary objective experiences are not parts of the really external world, they are pictures in our consciousness, or experience, of fragments of a homogeneous world which is really external to our experience and never comes within it at all. The conclusions of physical science are expressed in terms of our experience, but hold good symbolically of the world of things-in-themselves which forms the real environment of a human conscious experience. This doctrine meets the need, which "natural" realism can not meet, and which idealism does not try to meet, but which all men feel, of believing in a real, simple, homogeneous external world surrounding our narrow fields of experience. Whether the doctrine is true, and what the positive arguments are which support it, would be matter for another paper.

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THE FACULTY DOCTRINE, CORRELATION, AND EDUCATIONAL THEORY. II

V. THE METHODS OF SOLUTION

ALL doctrine which is to have pragmatic value, that is, which is to result in exhortations—the normatives rather than the normals of science—must issue in some definite practise which can be definitely tested. Unfortunately, the ordinary work of schools and colleges is so complex, and so many factors enter in as constituents to the results, that, though each side can claim the highest successes for its own doctrine, there is no evidence which opponents will accept. There is much valuable and interesting work already done by psychologists bearing on the issues previously mentioned; but they have mostly used units of work which educationists declare to be trivial and mechanical and therefore to have no or little bearing on the higher mental processes with which teachers are mainly concerned. It is argued further that to correlate the results of work in school subjects is useless, because no school subject is homogeneous mentally;

perception, memory, imagination, and reasoning are all involved and involved interdependently; and as one can not get one's units even approximately pure, the correlation of school subjects proves nothing scientifically. I ask psychologists if it would not be well to meet these objections by correlating certain mental processes within these school subjects themselves, using, doubtless, specially prepared exercises for the purpose, but still such exercises as may or can form part of ordinary school work.

First of all, then, we shall avail ourselves of the methods of correlation which are now readily at hand. If a mental process *A* is highly correlated with another mental process *B*, there is some presumption that a connection exists.

At the very lowest, such a relation enables us to infer from the quantified presence of the one to the quantified presence of the other; not indeed, inevitably, but with different degrees of probability. This is valuable knowledge both to the teacher and to the examiner; it will help the teacher in the classification of his children; and the examiner, if he knows the correlations, can cut down the number of separate tests which he will have to make.

But the teacher wants to know more, and he is apt to take the expression of a correlation as implying cause and effect. If some memory process, for example, were highly correlated with some process of imagination, he would suppose that, by training one, if it could be trained, he would be helping the pupil to improve in the other.

Is this reasoning justifiable? I fear not. Logically speaking, correlations are mostly between synchronous performances or attributes. But they *may* be sought for between antecedents and consequents, and if we can get them, however roughly, in the second form, we shall convince the educationist that experimental psychology has something to tell him of real importance to his work. The teacher is in search of the results of training in various functions, and of the results of the transfer of training, if any, from one power or faculty to another, from one subject of instruction to another. Correlations between coexistences are not all that is wanted; what is wanted also is direct experiment on the possibility of training and the possibility of transfer of improvement. And we must use tests which appeal to the educationist as having actual relationships to school work. What the teacher wants to know is the productive activity of his work, not merely the static relationships between the different parts of it. The early writers on correlation never overlooked the fact that correlations were not causes. Professor Karl Pearson in his "Grammar of Science" especially warns us that they are not. But there is a strong tendency, often realized, to regard correlation as

implying more than a relationship of mathematical function. It is taken to mean a real relationship of mental functioning. Nor is it merely educational theorists who make this error. In Professor Hugo de Vries's "Plant Breeding,"[1] there is a significant sentence, especially as, in other paragraphs of the same chapter, he has clearly said that correlation may imply coincidence merely. The sentence runs, "In all such cases there can be no doubt that the coincidence is a real correlation and that the cause which darkens the flowers is the same as that which darkens the seeds." But the correlation is just as real in the case of the coincidences as in the case of causal correspondences. It is the vague notion that correlation *means* causal connection which gives rise to sentences like this.

What, then, can the establishment of correlation do for us? We wish, for example, to find the relations between two mental functions or two groups of functions. First, let us deal merely with their coexistence. To make my observations more precise and perhaps thereby clearer to educationists, let us suppose we set out to measure the relationship between the accuracy of numerical computation and accuracy in arithmetical reasoning.[2] Expressed in teachers' language, the question runs: Do those who work their sums most accurately numerically also solve arithmetical problems better than those who are inaccurate numerically, that is, in actual addition, subtraction, multiplication, and division? That these powers may be sometimes disjoined we know, if M. Poincaré's evidence be accepted. "Quant à moi," he says, "je suis obligé de l'avouer, je suis absolument incapable de faire une addition sans faute," and he speaks of Gauss as an exception among mathematicians, for he was both a brilliant mathematician and "un calculateur très précoce et très sûr."[3] And I have heard more than once in educational discussions that Newton could not work a simple addition sum correctly. The statement is usually an excuse for inaccuracy in arithmetic. The implied argument is that accuracy in numerical computation is not necessary to mathematical genius and, shall we say, the expectation that little Tommy, who can not get his sums right, may, nevertheless, some day, become a mathematical genius. The argument would be more to the point if it could be shown that Poincaré's eminent mathematicians and Newton himself could not do their sums *when they were boys.* For men of capacity tend to be bored by the very early stages of a study whose limits they are seeking to extend. From personal reasons I feel quite impartial, for I am neither a good mathematician nor a good calculator. But let us leave this question of adult psychology; it

[1] 1907, p. 244.

[2] W. H. Winch, "Accuracy in School Children," *Journal of Educational Psychology,* December, 1910, pp. 557–590; and May, 1911, pp. 262–272.

[3] *Revue du mois,* July 10, 1908.

is not really germane to the issue. Back, then, to the children. We must measure the functions somehow and, if possible, without introducing artificial variants from the ordinary work of the child. Let us suppose we succeed in measuring one of them. How many measurements must we make? Suppose we make two or three, and endeavor to calculate from these a true value based on the conception that our measures are rather faulty, and that the variability in the measures is due to "errors of observation." If we do, we are running counter to the opinion which all persons hold who are daily and hourly in contact with the work of school children. They insist that the children themselves are varying from day to day and that the variability is in the exercise of the function and not merely in our observations of it. And there is a further difficulty; the functions, unlike physical measurements, will not stand practically still while we are sizing them up. We can not measure a function without producing a change in it, mostly an improvement. And, unfortunately, all children do not show the same rate of change, not even those who start initially at the same level—if we may call the result of the first two or three exercises a level in any true sense. May we say that the first few exercises are a measure of initial ability or natural endowment, and that the later results are measures of general educability rather than of native capacity in the one definite direction? I incline to think that the facts of correlation between two functions under different conditions may appear to lead in that direction. But there are difficulties in the way. Consider half a dozen people just beginning to play lawn-tennis. Is their native capacity to play lawn-tennis shown in their first game, in their second game, in their third game, in their first dozen games, or after their *n*th game? I suppose, as a matter of common sense, we should say that their "true form" is attained—we are considering these six players only and their relations to one another—when, with equal practise, we could place them in definite order from which, for some time, they do not vary. Jones does not always play the same game, neither does Robinson, nor Smith, nor the rest. But Jones can always beat Robinson and Smith; and Robinson can always beat Smith, and so on. I choose an illustration from the problems of sport because, among Anglo-Saxon peoples, much more anxious thought has been bestowed upon such topics than upon mere workaday affairs. What does such a view imply, if accepted as satisfactory in relation to mental measurements? Some such condition as the following would result. We should need to keep on measuring our function until our list became practically steady. The steadiness required would not be a plateau-like regularity—I am speaking in curves—of *A*'s successive results. Such steadiness is very difficult to obtain with young people, though, in a

few cases, by a careful arrangement of exercises as to difficulty and time, I have for certain purposes approximately obtained it. But indeed it is not needed to fulfil the condition required. We can rest satisfied that the true position of the pupil in our list of results has been attained when successive measurements do not cause any serious alteration in the relative position of the individuals tested. Expressed in mathematical language, the condition would be fulfilled when the coefficients of correlation between the successive results for the same group or list of children were highly positive. Let us, by and by, accept this position for a moment and see how it bears on much educational work on correlation—I am speaking, be it remembered, purely from an empirical standpoint, I make no claim whatever to deal with the mathematical theory of correlation. But before accepting it, let me illustrate the previous position by another modern instance.

Is there such a thing as general suggestibility? If there is, it is sometimes supposed that if we measure the suggestibility in two functions of a number of individuals, the double series thus obtained will have a correlation highly positive. If there is no or little correlation, we conclude that there is no such thing as general suggestibility. Admitting for a moment that these inferences are valid, how shall we set about obtaining our data and our conclusions? Is it satisfactory to take one or two measurements, say, of suggestibility to temperature sensation, and one or two, say, of suggestibility to visual after-sensations (the so-called visual images), and correlate the two series of results? I do not wish to prejudge the question of the existence of general suggestibility; but I wish to give some empirical reasons for doubting the validity of the method.

Suppose I take ten tests of rote memory for meaningless things one day with a class of children (a class of children approximately of the same mental level and taught always by one teacher), and then, a few days afterwards, long enough for fatigue effects to have disappeared, I take ten other tests of exactly the same character. I shall probably find the positive correlation to be no higher than .4 or .5. Am I to conclude from that result that my tests are of different difficulty and that the difference in their "content" has produced this low correlation? It is a difficult supposition, for reasons which will be apparent presently.

A week later I give a third series which, perhaps, correlates a little more highly with the second than the second with the first. Still the correlation is very low between functionings which appear to us introspectively precisely the same. Can we account for the low correlation by errors of measurement? Hardly; the differences in many of the individual results for set 1, set 2, and set 3 appear much

too large. Is it due to the functional variability of the children? Do they actually do different work on different days? We can readily suppose they might if we tested them in different conditions as to freshness and fatigue, if we repeated the tests at unequal intervals, or on different days of the school week, after different school exercises and different lessons. But if we are careful to avoid all these sources of low correlation, shall we still obtain the sort of results indicated above? I think we shall, in fact, with all the ostensible conditions equal. I have actually obtained the sort of results I have given above. But let us go on with the experiment: a fourth series is given, a fifth, a sixth, and more. Presently there comes a time in which each pupil settles down, so to speak, in his place, and the correlation between two of the later series may rise as high as +.9 (Pearson "r" coefficient) and with rank formulæ may rise to unity. This fact would seem to indicate that the initial variability was not due to the differing difficulty of the earlier series one from another, but to a true functional variability on the part of the children: also, that our methods of measurement were not very faulty, for, by a continuation of the same methods, we at last achieve some series of steady results.

We may now regard ourselves as possessing a reliable measure of the function for the period in question. And if we have in similar ways measured another function throughout the same period, we are in a position to find the correlation between them. Precisely what conclusions we may draw from the absence or presence of high correlation between series thus obtained is a question which, for the present, I leave on one side. I emphasize only the consideration that, from the series obtained from one or two exercises (especially those of a novel character) in each of two functions, we can not, at least with school children—I speak empirically only, and of the results with which I am personally acquainted—we can not, I say, hope to find a high positive correlation; for the earlier series, obtained by measurements of *the same function with the same "content,"* do not correlate very highly among themselves. For practical purposes we can obtain a correlation which will serve if we take the totals or averages of three or four sets of tests (given under the conditions referred to above) in each of two functions; but, strictly speaking, we may be fairly certain that our correlation is too low unless we have first obtained steady measurements in the functions themselves. Though then, with adults, there *may* be little of what can be called general suggestibility, still it does not seem that we are entitled to a negative conclusion without more certainty that the measures of the separate functions are reliable.

There is another aspect of the case which I may fairly be charged

with having neglected up to the present, namely, specialization according to subject-matter. Let me illustrate the point by means of an example drawn from experimental work on memory. Suppose we are dealing with immediate rote memory for meaningless things: we set out to test whether the power to remember unassociated and meaningless figures is highly correlated with the power to remember unassociated and meaningless letters. But what precisely are we trying to find out? Are we expecting to find what we want by appealing to cases in adult life in which there may have been a long specialization preceding our experiments? Some of our adult subjects may have done a good deal of linguistic work and the memory of the juxtaposition of letters and words, at first, *bien entendu*, a meaningless juxtaposition with associations only of time and place, inevitably plays a part in all linguistic acquisition. Others may have worked much at a kind of mathematics in which memory for juxtaposed figures plays a considerable part, as, for example, the memory for roots, squares, and logarithms, not to speak of other memorized statistics required in most branches of applied mathematics and science. We can, of course, if it is thought justifiable, find out the correlation between *any* two series of results. We might get some notion of the influence of training if we knew beforehand the probable mnemonic values for persons of the same mental level without training. And that would indeed be valuable. But we usually suppose something rather different. We often tend to think implicitly that, when we have found correlations between two mental functions of a set of students, however variously trained, we have found out a value bearing directly on the truth or falsity of the faculty doctrine, and that we have discovered a value of directive service to pedagogy.

There are, I think, some considerations which make these opinions very doubtful. First of all, the faculty doctrine, except in a form which I attack rather than defend, does not require us to assert that, after various and diverse training, a set of students will be found in whom the positive correlations for these various rote memories for meaningless things are very high. The only form of the doctrine with which I am concerned asks about the relation of potentialities rather than of completed products. I do not for one moment intend to take refuge in mere potentiality. Like other experimenters, I measure potentiality by work done; but, as far as possible, I wish to avoid asserting potential relations when very different amounts of work have been done in the two functions which I am trying to correlate.

Let me put the matter more bluntly and from a pragmatic standpoint. The educationist wants to know whether there is not a kind

of indeterminateness about this rote memory power. He wants to know whether, with equal training in both directions—the learning of letters and the learning of figures—there would not be found, usually, and on the average, a high positive correlation between the two functions. If we work with school children who have learned practically nothing at home, and measure only such children as have been exposed to similar educational environments in school, identity of curriculum, and of methods of instruction, we have a much better chance of discovering the relations of the potentialities. Calculating boys are hardly to the point, they are already specialized. We know that we can use up this memory power, for, by giving exercises hard enough and often enough, we can produce a condition in which, for a time, until fresh growth has occurred, we can obtain a descending series of results. Is it not reasonable to suppose that a long process of training in, say, the memorization of numbers will throw the mnemonic function out of balance and so give us results which, for a time at least in the young, and relatively permanently in those in whom the growth potentialities have been mostly realized and in whom growth is near its limit, will show high values for the results in the memory for figures and low values for the memory for letters? If this is the case, the correlation between the two series of results, when individuals differently trained are the subjects of the experiment, will not be very high even if the improvement of the one function produces some improvement of the other within the same mind.

There is a further condition necessary to success if we are trying to measure the correlation between two mental characters. Let me illustrate as before by means of the rote memory for figures and for letters. If we give so many letters for one test, we must give the same number of figures for one test. If we consider the accuracy or otherwise of the order of the figures, we must do the same with the letters. If we arrange the letters in rows of a certain number, we must do the same with the figures. If we allow a certain time for the memorizing of the letters, we must do the same with the figures. And we ought to measure individuals who have had no specialism in their training. Also we should measure the functions under equal conditions as to the subjects, freshness, and fatigue. If, for practical purposes, we are unable to fulfil these conditions, we must be prepared to accept our coefficient of correlation with the full consciousness that it is probably too low.

Suppose we make the functional conditions the same and are careful to take enough measures of each individual's work, we shall find, I venture to suggest, as far as school children are concerned, a number of rather high positive correlations between most of the quali-

ties which we are accustomed to consider as intellectually good. Let me assume this result and ask what it implies.

VI. High Correlation and Functional Connection

May we infer functional connection from the presence of high positive correlation between two functions?

Shall we, from the general establishment of high correlation, find ourselves back again in the full flood of the faculty doctrine, nay, even more, back to a belief in the approximate unity of all mental function? The mathematical theory of correlation—I speak as a scribe or pharisee, accepting the authority of others—does not require us to go so far; and on empirical evidence, I should decline to go. Let us look at the logic of the question for a moment.

Two functional quantities α and β appear to vary together, and let us suppose that the establishment of high correlation of α and β comparatively for a number of different individuals has entitled us to assert that α and β vary together within the same mind. One function may cause another; they may both be dependent upon a common cause, or they may be merely coexistences. It is hard to believe the latter, though one school of psychologists and philosophers in the doctrine of psychophysical parallelism are pledged to it. If they depend upon a common cause, it may be one which is inaccessible to our operations. But our method would be to *try* to find it, operate upon it, and see if it produced these correlated variants α and β. Of course, if they were causally related we could operate on each and see if we could thereby produce variants in the other. It seems obvious that high correlation sets us a problem of connection. It does not *ipso facto* enable us to conclude that a relation of interdependence exists.

VII. Low Correlation and Functional Connection

Among educational psychologists, as I have said, it is frequently believed that a high correlation between two series of measurements in different individuals indicates that there is such a connection between the measured functions that growth in one is correlated in a highly positive way with growth in the other; and that improvement produced by training in the one is transferable to the other. Professor Thorndike, a long time ago (I think in the *Psychological Review*), pointed out to educationists and psychologists that these conclusions do not follow from the existence of positive correlation, and I have dealt with these problems in a preceding section.

But I now approach an issue in which I believe educational psychologists to be unanimously against the opinion which I shall hesitatingly suggest. I am going to advance the possibility of functional

connection with low correlation. I speak always, be it remembered, of the mental powers of school children, and I speak always empirically. Suppose we measure a number of individuals in two functions, say, in rote memory for meaningless things and substance memory for stories.[4] We give three or four tests or more in each function, and find the correlation between the total results of the measurements of the two functions. With school children of nine, ten, and eleven years of age, this correlation is found to be decidedly low, perhaps scarcely twice as great as the "probable error." Yet practise in rote memory for meaningless things will, with these children, produce a decided improvement in their substance memory for stories. That is, there is a real connection between these functions though the correlation is small or doubtful. How can this be?

I make the following suggestion very tentatively. We are accustomed to measure the correlation between two functions *in the same mind* by seeing what relations of magnitude exist in functions α and β possessed by Tommy Brown, as compared with the magnitudes of the functions α and β possessed by Charley Smith and Johnny Robinson and the rest. Let us suppose that Brown remembers stories better than Smith or Robinson, but meaningless things badly, indeed worse than Smith or Robinson. Smith is intermediate between Brown and Robinson both in memory for stories and memory for meaningless things; and Robinson is best at meaningless things and worst at stories. Arranging in order (of course our cases are much too few in number except for illustrative purposes) we have:

	Rank in Memory for Stories	Rank in Memory for Meaningless Things
Brown	1st	3d
Smith	2d	2d
Robinson	3d	1st

There is obviously no positive correlation when we compare one individual with another. Must we therefore conclude that there is no correlation of the two functions within the same mind? Brown's capacity may be big in one direction and small in another *as compared with that of Smith and Robinson,* but an alteration in one of his functions may produce some alteration in the other. To find the connection of functions within the same mind, would it not be best to get a number of measures for the same individual and correlate these?

We can not, however, simply take a number of measurements of function α and then a number of measurements of function β in the

[4] W. H. Winch, "The Transfer of Improvement in Memory in School Children," *British Journal of Psychology,* December, 1910.

same individual and correlate these without further consideration. For there is this further difficulty in the measurement of mental functions, especially in the young. The process by which the measurements are obtained will, almost certainly, produce a variation in the same direction in both functions. The second measurement of function α will give, generally speaking, a higher number than the first, the third higher than the second, and so on. And similarly for the successive measurements of function β. So that we may have produced a sort of correlation by our measurements. We may call this a correlation of improvability. Doubtless, some sort of common factor may be involved, even as vague a one as intensity of attention. How far intensity of attention can operate equally upon two different original endowments in two functions, one large and the other small, and how far it depends for its intensity of operation upon those endowments, are questions which I leave aside at present. We should, in young people at least, be almost certain to find two series of increasing numbers which would give a positive coefficient, even though the functions are not correlated when measured against those of other individuals. It may be interesting to the teacher to know that both functions improve with practise; he probably knew it before. But that is not really what he wants to know. He wants to find real connections directly between the two functions themselves. That is what he understands the faculty doctrine to mean. Perhaps these real connections may exist in the same mind even if there is low or doubtful correlation of the functions α and β when one individual is compared with others. And perhaps that real connection may not exist when positive correlation is found between two series of measured results of two functions α and β for the same individual. How then can teachers find out what they want to know? We seem at an *impasse*.

Let me pursue the matter a little further. Suppose we take Tommy Brown and make a few measurements of function α and a few measurements of function β. If the results are fairly steady we may consider that the totals or averages of these results are a sufficiently accurate measure for our purposes of his capacities in functions α and β. Train function α by additional exercises; and let us suppose we produce an improvement upon his original work in function α. Now measure function β again. We shall be almost certain to find an improvement. May we set this down to the transfer of improvement from the training of α? Certainly not, unless we know how much is due to the natural growth of the function β, for Tommy is young, and is growing all the time. And how can we find that out? Pending the day when we shall have enough measurements under different conditions to give us approximate values

for things of this kind, we must have "control" cases who have not been trained in function α to compare with Tommy; and this brings us to the method of "equal groups." By this method, one of two groups (of tested equality in function β) is trained in function α, the other is not trained. After a time both groups are again tested in function β. The excess improvement (if any) of the trained over the untrained group may be taken (subject to certain statistical conditions) as a measure of the transfer of improvement. We suppose that, during the period intervening between the preliminary and final tests of function β, the natural growth of the two groups in function β has been approximately equal, and this natural increase may account for a considerable portion of the improvement in both groups. It is, as I have said, only the excess improvement of one group over that of the other which we consider a transferred improvement.

Such a transfer of improvement means, if we have made our measurements properly, a direct relationship between functions α and β. They are related functions; improving α implies the improving of β; they form, in an intelligible and verifiable sense, part of the same faculty. The mental functions thus connected will, I believe, give "groupings" or "faculties" rather unlike those of early psychology.

VIII. A Modified Faculty Doctrine and its Relation to Pedagogy

Suppose that, finally, we are not entirely deprived of our faculties; suppose that the progress of experimental research, with more continuous tests and a clearer understanding of what formulæ of correlation can and can not do for us, restores us some of them, though perchance in a different form; what then?

Are we to plunge once more into all the errors consecrated by the old meaning of the term *Formale-Bildung*? Are we to believe, in English phraseology which was once very current, that the education of a scholar and a gentleman sufficeth for all things? Surely not. The answer we must give will depend on the quantitative relations of the improvements obtained by training related functions.

We can train memory? Yes, but to learn the things we need to know (I am speaking only of those things for which memorization is a suitable method) will train our memory as well as give us useful acquisitions. In order to give us justification for purely formal methods we should need to show that, by the formal training of function α, we can produce a transferred improvement in function β which we could not, with equal work, produce by training function β itself.

It is function β we want and not function α. If we can get more of β by working on α than by working on β directly, well and good; let us work for β by training α. I do not wish to deny that cases may exist in which this is possible, but they require experimental proof to determine them, and they are surely very rare cases.

In England, a few years ago, it was very customary, in elementary boys' and girls' schools, to train for drawing from the wrist by drawing first from the shoulder (free-arm drawing). The two functions were part of the same group of functions, they were part of the same educational faculty—the faculty of drawing. The procedure was quite in line with other inferences deduced from the faculty doctrine. No educationist thought it necessary to show that, with normal classes of senior children, the transferred improvement was greater than the improvement produced by a direct attack. Yet such a result is always demanded before the pedagogy of faculty-training is justified.

There is, however, another general consideration. Function β may not be accessible to direct attack. Well, if it is not, our problem is more difficult, but is solvable along the same lines. We must try our probably related functions, keep our records of improvements, and see which transfers most to β. But suppose β is practical efficiency in a trade or profession. Well, even then, we must try to get data of success or failure and compare with preceding educational practises. The results of such inquiries might, I fear, be painfully illuminating to many of us educationists; but if the balanced contentions advanced throughout this paper be sound, such a method is imperative if, in educational theory, we are to do more than oscillate violently between one extreme and another.

One cheering conclusion seems to me likely to issue as the outcome of work on the methods I have advocated. It may, I think, be found that education in the things we need to know for practical purposes, learning, imagining, and reasoning about them, will itself supply much of the training which a modified faculty doctrine would warrant.

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DISCUSSION

A PROTEST

DR. HOLLINGWORTH'S review of my "Text-book of Psychology" contains the following passage (this JOURNAL, Vol. VIII, p. 274):

> The two-level theory [of attention] is deliberately expounded in the text, although hidden away in the references for further reading is the confession that

the author "has fallen into the common psychological mistake of generalizing his own experience" (p. 302). The "consolation that the multi-level modes of attention were first observed" in the author's own laboratory seems hardly sufficient justification for concealing them from the student.

The first half of the "Text-book," including the chapter on attention, was published separately in June, 1909; the final proofs left my hands some time before I learned of Dr. Geissler's observations. As soon as these results were brought to my notice, I confessed my mistake ("Thought-processes," November, 1909, 292 f.).

I felt it imperative to make a like confession in the complete "Text-book" (1910). The only available space was a blank on page 302, which gave the "references for further reading" on attention. I therefore inserted forward-references in the text (pp. 278, 290), and on page 302 added the paragraph from which the reviewer quotes. The fact that Dr. Geissler's work appeared after the chapter on attention had been written is indicated, a few lines further down, on the same page.

A thorough revision of the "Text-book" would require the rewriting, not only of Section 77, but also of several other sections. Meantime, Dr. Hollingworth has offered me a public insult. I expect a no less public apology.

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REPLY TO PROFESSOR TITCHENER'S "PROTEST"

FROM one point of view it is unfortunate that a reviewer of a text-book is compelled to discuss his material in the condition in which it comes to his hand, as a finished product. The situation is particularly embarrassing when the mechanical difficulties afforded by the printer's layout render impossible modifications which the author would much prefer to have made. The book referred to in the above "Protest" reached the reviewer in the fall of 1910, unaccompanied by any indication that an earlier electrotyping had made the final form of the text unsatisfactory to the author himself. The supplementary modification (p. 302) of the section dealing with the two-level theory of attention seemed to the reviewer to detract from the serviceableness of a book in other respects so excellent that it at once became a standard reference for his own students. This was an honest impression, and it was recorded with that fidelity to introspection for which the "Text-book" pleads. Professor Titchener's explanation that the reason for the arrangement was mechanical necessity rather than deliberate intention is an adequate one. But it is none the less to be regretted that the mechanical necessity was present and that its mechanical character is not obvious to the

reader. If Professor Titchener considers the previous expression of this feeling to be a public insult I beg him to accept this paragraph as an equally genuine public apology.

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REVIEWS AND ABSTRACTS OF LITERATURE

The Evolution of Mind. JOSEPH MCCABE. London: Adam and Charles Black. 1910. Pp. xvii + 287.

This is a book which the reviewer finds difficult to estimate justly, because of the tone in which it is written. The author, it may be remembered, was formerly a priest and a professor of neo-scholastic philosophy, but has now wholly abandoned the theological for the scientific view of the world. He does not seem, however, to have put on the modesty of the true man of science. His general spirit will perhaps be indicated by some quotations from the introduction. After stating that his main problem is whether mind can be brought into the unity of physical evolution, he goes on to say: "My aim is, in short, to bring together whatever facts may be found to bear on the subject in a dozen sciences—chiefly physics, organic chemistry, geology, paleontology, zoology, physiology, psychology, and anthropology—and enable the reader to see whether the great advances which have recently been made in these branches of science have brought us any nearer to a verdict than we were in the days when monists, dualists, and parallelists fought their historic battles. . . . Modest [*sic*] as is the design of making such a synthesis, and although the author has personal acquaintance with more than one stage of the inquiry, it has entailed so much labor that a word of personal explanation should be given. . . . I have, as the works indicated will acknowledge, sought aid in the whole relevant literature of Europe and America."

It will be seen that Mr. McCabe lays claim to an erudition little short of stupendous. Of the validity of this claim, so far as the majority of the sciences above enumerated are concerned, the reviewer is imperfectly qualified to judge: the author seems to deal with facts and theories in an easy and masterful way quite dazzling to the uninitiated and possibly to the initiated as well. When, however, he comes to the facts of comparative psychology, which naturally form the main bulk of his evidence, it becomes more comprehensible how one small brain can carry all he knows. The following list seems to represent the authorities consulted by the writer in this field: the works of Lloyd Morgan and Romanes, Jennings's "Contributions to the Study of the Lower Organisms" (not his later work), Verworn, Loeb's "Comparative Physiology of the Brain" (nothing else of Loeb's, apparently), Hobhouse, Darwin on the earthworm, Preyer on the starfish, one paper of Rádl's and one of Rhumbler's, Dahl on the spider, some (by no means all) of the work of Bethe, Wasmann, and Forel; Mr. and Mrs. Peckham's popular book on wasps (not the monograph), Whitman's essay on animal behavior,

and Thorndike's "Animal Intelligence." He implies that Thorndike has done no work on the primates. Of von Uexkuell, whose writings would certainly seem to belong to "the whole relevant literature of Europe," Mr. McCabe has had an intimation: he calls him "von Uexhuell," throughout, and regrets that he has been unable to trace a paper of his quoted in the "Cambridge Natural History." He is apparently unacquainted with any experimental evidence for modifiability of behavior in the Protozoa or Cœlenterates, and the work of Yerkes is totally unknown to him.

The conclusions which are reached on the basis of the facts thus gathered may be briefly summarized. The only evidence of the presence of mind on which the author relies is the anatomy of the brain. "The only reliable . . . way to infer consciousness is from the structure of the brain." Learning by experience is rejected as a criterion because it occurs unconsciously in human beings. "No one will suggest that the more elaborate nerve processes which underlie our higher conscious states may be found in the animal without consciousness, but there is no reason why the simpler associative processes may not be so found. Unconscious inference is common enough in human experience. The rapid processes of reasoning which superficial writers call 'intuition,' especially in the case of woman, are largely unconscious." The appearance of the cortex marks the beginning of consciousness in the evolutionary series, the stages of which are traced with reference to geological changes in a way which makes interesting reading. Upon the main problem of the book, "whether a new reality or agency beside ether intervenes at some point in the earth's history," a negative outcome is reached. "Frankly, I find no speculation on the origin and nature of consciousness that is worth reproducing, and I assuredly have none to offer. . . . It seems to me quite hopeless to speculate on the origin of consciousness as long as its organ is so wrapped in mystery. . . . At present our knowledge of the cortex, the most transcendently important thing that science approaches, is appallingly meager." Why, then, one may ask, be so sure that mind is impossible before the cortex develops?

The last chapters treat of the progress of mind after its appearance: the most important factors in this progress are held to be the substitution of ground-dwelling for tree-dwelling habits, the successive advances of the ice-sheet, and the great migrations and consequent intermingling of tribes. Whatever the shortcomings of the book, it holds one's attention throughout by its lively style.

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The Unexplored Self: An Introduction to Christian Doctrine for Teachers and Students. GEORGE R. MONTGOMERY. New York and London: G. P. Putnam's Sons. 1910. Pp. viii + 249.

Although all religions of the inward type have been individualistic and empirical, theology has commonly preferred to deal with the universal. Of late, however, the empirical emphasis has invaded even this

cloistered realm, a fact of which Dr. Montgomery's little book on "The Unexplored Self" is one of the most recent indications. It starts avowedly from personal experience, and finds the warrant for its larger constructions in the implications of that experience. On the first page the question as to the worth of a man is raised—an inquiry which carries one at once into the heart of theology, for worth is as distinctly the key-note in constructing a harmony of the future as cause is in constructing a harmony of the past. The author does not argue for his own (Christian) answer, that men are of high present worth, with "unlimited possibility of growth in value"—younger brothers of Christ—but calls attention to it, and sends the reader to his own personal experience for its corroboration. If one does not find an adequate confirmation there, the counsel is to enlarge the personal activities in lines calculated to test the hypothesis of human lasting value; for, he says, "one who plunges into the purposeful activities of society finds the sense of a final purpose (future values) coming as a matter of course." That is, let one explore himself, and bring to consciousness new parts of his nature, and see if they do not harmonize with and confirm the hypothesis in question. This certainly seems to be the logic of Jesus, also (*cf.* John 7:17), but it is a line of inquiry which is matter for scorn to a certain type of philosopher. What have a man's inner experiences to do with the constitution of the universe? The test-tube and the spectrum must decide as to that. Full many of us are evidently in a transition stage as to man's relation to the cosmos. He has been shorn of his ancient dignity as a kind of demigod, a child of heaven and earth, and yet he is still regarded by many as too separate from the cosmos to be treated as a veritable phenomenon, a disclosure of its nature. The author is not guilty of this inconsistency. For him discoveries made in the "unexplored self" are discoveries in nature in general, and replete with implicit revelation. "Analysis," he tells us, "of any or of all experience discloses the two functional variables, value and relation." Reasoning, which he does not disparage, "can not originate the value." It is an empirical datum, in which the self is the cardinal factor. Indeed, the "assurance of reality that comes from preciousness . . . has been perhaps more provocative of discoveries" than "that which comes from inferences."

So, too, the divine rests upon the instinctive functioning of the self at its best. Wonders, whether miracles or natural mysteries, are no proof of it. "There has ever been a perversity in human nature which would equate unknown and divinity. That was Herbert Spencer's primitivism." On the contrary, the divinity of Christ is thoroughly natural, "if by natural we mean those events that fit vitally into human experience." That so often it does not seem so is due to the fact that men are wont to frame a "pompous, magniloquent," unproven definition of God, and then say, "God we have found out about; now, was Jesus his son?" whereas the real problem is, "Jesus we know; now, did he have a heavenly father?" "We reach deity through the highest human." Similarly it is not nature, but life, that witnesses to immortality. Faith

is essentially a proper valuation of the weaker lights of experience, which the glare of coercive reasoning too often shuts out.

The reader will find in these suggestive pages, with their quiet Nazarene style, many familiar (and challenged) teachings set forth in new and persuasive aspects. The least satisfying chapter is that on "The First and Great Commandment." The primacy of love to God is maintained properly enough, but the first reason for it (which should be the main one), that "there is much more to life . . . than merely loving one's neighbor," is left in negative and hence unconvincing form. Its content is not given, nor is the "much more" suggested. Why does not some Christian (or Hebrew) ethicist develop the content of the commandment of supreme love to God, and bring his ethical system into touch with that of the Greeks by showing that that commandment really calls for the highest individual aspiration, and that its upshot is the validation of man's creative faculties—his highest individual functioning?

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JOURNALS AND NEW BOOKS

THE INTERNATIONAL JOURNAL OF ETHICS. January, 1911. *William James as Philosopher* (pp. 125–153): A. O. LOVEJOY.—The more specific significance of James consists in this, that he brought to the vocation of the philosopher an almost unequaled power of seeing these two generic aspects of reality: the uniqueness and inwardly self-authenticating character of concrete existences, and the uniqueness and the primacy of the temporal quality of experience. *The Place of Leisure in Life* (pp. 153–165): B. BOSANQUET.—A popular account of the unity or plot of Aristotle's "Ethics." Leisure is the satisfaction of a disinterested interest and is, for Aristotle, the goal of education. *Charity Organization* (pp. 165–178): THOMAS JONES.—A reply to Bosanquet's criticism in the *Journal* of July, 1910, and a statement of the theory of pauperism. *Goethe's Influence on Carlyle* (pp. 178–189): F. MELIAN STAWELL.—The first of two papers in which is noted the emphasis in Goethe upon the kinship and sympathy between man and nature and the necessity of renunciation for the higher life. *Idealism and the Conception of Forgiveness* (pp. 189–198): J. W. SCOTT.—Badness is defective synthesis, and this is not an essential element in human nature. Hence, as capable of elimination, and when eliminated, it may be forgiven. *Confessions of an Indeterminist* (pp. 199–215): W. F. COOLEY.—Free will is a multiform, but essential, susceptibility to values, a susceptibility not rigidly fixed but subject to change apart from circumstances. In this sense it is essential to individuality. *Reviews:* F. W. Bussill, *Marcus Aurelius and the Later Stoics:* T. WHITTAKER. Walter L. Sheldon, *The Story of the Bible from the Standpoint of Modern Scholarship:* N. SCHMIDT. R. L. Otley, *Christian Ideas and Ideals:* DAVID PHILLIPS. John W. Buckham, *Personality and the Christian Ideal:* G. R. DODSON. Marion Le Roy Burton,

The Problem of Evil: THOMAS C. HALL. Stuart H. Rowe, *Habit Formation and the Science of Teaching:* GUY WHIPPLE. Edouard Roehrich, *Philosophie de l'éducation:* W. J. ROBERTS. M. W. Keatinge, *Studies in the Teaching of History:* M. L. EASTWOOD. Charles Werner, *Aristote et l'idealisme platonicien:* S. WATERLOW. Síttánáth Tattvabhúshan. *The Philosophy of Bráhmaism:* J. S. MACKENZIE.

ARCHIV FÜR GESCHICHTE DER PHILOSOPHIE. Band 22, Heft 1. October, 1910. *Die Zweiteilung in der Terminologie Heraclits* (pp. 1–21): E. LOEW. – Two series of terms are grouped in opposition to each other about λόγος as the false, and φρόνοις as the true way of knowing. *Die Entwicklung der Geschichtsphilosophie W. von Humboldts* (pp. 22–60): L. EHTEN. – Humboldt at first taught that each individual man is the expression of a definite, unique idea, new and unclassifiable; whose perfection, however, depends wholly upon his moving towards the universal man-type; which movement can only be expressed esthetically. But later he looked on the idea as wholly dependent on the original activity of the individual. So that the historian's task is primarily to present how individual men realized their purposes, not to discover the goal of historical movement. *Nachlese zur ältesten Geschichte des Spinozismus* (pp. 61–98): DUNIN-BORKOWSKI. – Some Spinozistic *"Munchhausen"* and *Märchen,* with two innocent Dutchmen who were converted from a mistaken opposition to a no less mistaken approval of Spinoza's doctrine. *Die Monadenlehre in ihrer wissenschaftlichen Vervollkommnung* (pp. 99–127): E. RAFF. – Sixteen principles underly the monadology which established "ideological monism," the monads being the ultimate elements, and the substrate of thinking. Sixteen deductions are made from these, chiefly in regard to the subject-object relation. *Rezensionen.* A. R. v. Kleemann, *Die Stellung des Enthyphron im Corpus Platonicum:* C. FRIES. E. Caird, *Die Entwickelung der Theologie in der griechischen Philosophie:* C. FRIES. N. Hartman, *Plato's Logik des Seins:* C. FRIES. *Die neuesten Erscheinungen. Historische Abhandlungen.*

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NOTES AND NEWS

At the meeting of the Aristotelian Society on May 29, Miss E. E. Constance Jones read a paper on "A New Law of Thought." In every proposition of form *S is P*, S and P denote the one thing (SP)—the *is* therefore signifies identity of denotation; extensionally or denotationally *S is P*—and the attempt to interpret *S is P* in extension only would reduce us to *S is S:* difference of intension of the term is necessary for significant assertion. And we can not "identify" the extension or denotation of the one term with the intension of the other. And in intension *S is not P.* We can only say with Lotze that, taken in intension, *S is P* is impossible, and must be resolved into S is S, P is P, S is not P. *S is not P* asserts difference, or otherness of denotation in intensional diversity, *i. e.,* it denies what *S is P* affirms. It is not until S *is* P, S *is not* P, have been admitted and justified that we are entitled to formulate the law of contradiction and the law of excluded middle, and to say that

S is P, S is not P } can not both be true (law of contradiction) (1).
S is P, S is not P } can not both be false (law of excluded middle) (2).

Taking (1) and (2), together with the analysis of *S is P* into an assertion of identity of denotation in diversity of intension, we can say that of any subject (S) P must be affirmed or denied, *i. e.,* that of any subject (S), *P* or *not P* (but not both) may be predicated. Thus we obtain as a law of significant assertion the following formula: Any subject of predication is an identity of denotation in diversity of intension. If *S is P* (SP) is analyzed as above, immediate and mediate inferences are at once justified. Mr. Russell's criticisms of this view may be answered by pointing out (among other things) (1) that his proof that, *e. g., the author of "Waverley"* means nothing, seems to depend upon a double use of the word "meaning"; (2) that if in *The round-square is contradictory* the subject of the proposition has no denotation, this "proposition" (which Mr. Russell regards as a possible one) has no meaning of any sort—in fact, is not a proposition at all, and raises no difficulty whatever; for unless the roundness and squareness are referred to one thing (have one denotation), there is nothing self-contradictory in the subject; and moreover, if there were, it could not be asserted in the predicate, since *round and square* differ *in intension* from self-contradictory, and the whole thing is perfectly incoherent.—*The Athenæum.*

The Huxley lecture at the University of Birmingham was delivered this year, on May 29, by Professor Henri Bergson. He chose "Life and Consciousness" as the subject of his lecture. He proposed the view that a world-wide antagonism exists between matter, which is essentially automatic and governed by necessity, and consciousness, which is characterized by the power of choice and creation. Consciousness, he believed, entered matter in order to entice it to organization; but in thus binding itself to matter, consciousness lost much of its original liberty, and was continually being dogged and cramped by automatism. Professor Bergson conceived consciousness as flowing through matter much as a stream of fluid flows through a tunnel; in digging galleries in this hard rock,

consciousness found itself impeded at point after point. Thus it had repeatedly to make fresh starts in its ceaseless efforts to create, until finally, in the course of this crooked path of evolution, it created man. In man alone, the chains which elsewhere bound consciousness to matter were broken. Here, maintained the lecturer, every contracted habit could be opposed by another habit, every kind of automatism by another automatism. Consciousness thus acquired its liberty by setting one necessity to fight another. In this way it has expanded to such a state of freedom that in man, perhaps, it may be able to endure beyond his earthly life.—*Nature.*

In the School of Education of the University of Pittsburgh the following appointments have been made: Henry Davidson Sheldon, dean of the School of Education in the University of Oregon, has been made professor of the history of education. Dr. Sheldon will spend next year in Europe on leave of absence and will take up his work in Pittsburgh in the fall of 1912. Charles Barr Robertson, director of the schools of practise and professor of psychology and education in the Cortland, N. Y., State Normal School, has been called to the professorship of secondary education, and will organize and direct the practise teaching and the cooperative relations of the high schools and the university.

At the Bryn Mawr College commencement announcement was made of a bequest of $150,000 from Phoebe Anna Thorne, of New York, who died in 1909, to endow an associate professorship of education and the Phoebe Anna Thorne Model School to be conducted by the college as an experimental high school in connection with a graduate school of education.

The June announcement of the Macmillan Company includes "Truth and Reality," by Professor John E. Boodin; "The Essentials of Psychology," by Professor W. B. Pillsbury, and "Animal Intelligence: Experimental Studies," by Professor Edward L. Thorndike.

"Crime, Its Causes and Remedies," by Lombroso, forms the third volume of the Modern Criminal Science Series issued by Little, Brown & Co.

Dr. Arthur Mitchell, who for the past year has occupied the chair of philosophy at the University of Oregon, has been appointed assistant professor of philosophy at the University of Kansas.

A translation of "Pragmatism" by Professor Wilhelm Jerusalem is announced by the publisher, Dr. Werner Klinkhardt, of Leipzig.

The Fifth International Philosophical Congress will hold its meeting in the buildings of the University of London in the spring of 1915.

Professor G. P. Adams, assistant in philosophy at the University of California, will be absent on leave for the year 1911–12.

Mr. G. E. Moore has been appointed lecturer in moral science at Cambridge University, England, for five years.